naturally expressed thus, $V = L/T$. On the same principle we should have—

$$A = V/T = L/TT, \quad U = MV = ML/T, \quad \Delta = MA = ML/TT,$$
$$E = L\Delta = LML/TT.$$

This may be simplified, using the analogy of the fluxional notation, by writing $\dot{L}$ for L/T and L for $\ddot{L}/TT$. Then $\dot{L}$, $\ddot{L}$ may take the place of V and A, and we have—

$$U = M\dot{L}, \quad \Delta = M\ddot{L}, \quad E = L\Delta = LM\ddot{L}.$$

Names for the several units are hardly needed in the general system.

In the C.G.S. system the units of length, mass, and time are respectively the centimetre, gramme, and second, denoted by C, G, S. Then if U_G, Δ_G, E_G denote the units of momentum or impulse, force, and energy or work, we have, on the same principle as before, $\dot{C}$, $\ddot{C}$ to denote the speed of a centimetre per second and the acceleration of a centimetre per second per second respectively, and the equations—

$$U_G = G\dot{C}, \quad \Delta_G = G\ddot{C}, \quad E_G = C\Delta_G = CG\ddot{C}.$$

If *names* for all these units are required, we may use these: *vel, cel, mom, dyne, erg;* and we may say, a *mom* is a *gramme-vel*, a *dyne* is a *gramme-cel*, and an *erg* a *centimetre-dyne*.

In the F.P.S., or British system, the units of length, mass, and time are respectively the foot, pound, and second, denoted by F, P, S. Then, if U_P, Δ_P, E_P denote the units of momentum or impulse, force, and energy respectively, we have, on the same principles, $\dot{F}$, $\ddot{F}$ to denote the speed of a foot per second and the acceleration of a foot per second per second respectively, and the equations—

$$U_P = P\dot{F}, \quad \Delta_P = P\ddot{F}, \quad E_P = F\Delta_P = FP\ddot{F}.$$

I propose as names for these units: *footvel, footcel, poundem, poundal, pounderg.* I should have called the unit of force a *pound-dyne* or *poundyn*, but that *poundal* has already obtained general acceptance.

The foregoing are all absolute units. The corresponding (Greenwich) gravitation-units are the *pound-weight* = 32·19 *poundals*, the *foot-pound* = 32·19 *poundergs*, and the *second-pound* (as it has been proposed by Prof. Unwin to name the gravitation-unit of momentum, or time-integral of a pound-weight through one second) = 32·19 *poundems;* so that to convert absolute F.P.S. units into gravitation-units, or *vice versâ*, it is only necessary to divide or multiply by 32·19, since the acceleration due to gravity at Greenwich = 32·19 $\ddot{F}$.

It is, I think, comparatively unimportant whether the *names* above suggested are, all of them, accepted or not; but the *notation* will, I believe, be found a great aid to the beginner in fixing in his mind the dimensions of the different magnitudes, and an effective safeguard against the too common confusion of units of force, impulse, and work. I doubt whether in speaking much would be gained by saying "*footvel*" instead of "foot per second," or "*footcel*" instead of "foot per second per second," while in *writing*, the symbols $\dot{F}$ and $\ddot{F}$ might be always used and read either way.

ROBT. B. HAYWARD

Harrow, April 12

Units of Weight, Mass, and Force

FAR be it from me to interfere between Mr. Alfred Lodge and Prof. Greenhill; but, whilst leaving Mr. Lodge to his fate and Prof. Greenhill, perhaps, as an engineer, I may be permitted to offer a few remarks on the general question. First, then, it appears to me that Prof. Tait's thoroughgoing condemnation of certain phrases of the engineering vernacular, and of the grave errors of certain writers, has been strained in some quarters to mean a general charge against engineers of inability to think or write clearly on the physical laws which lie at the root of their every-day practice. Such an unqualified charge is on the face of it absurd; for otherwise we must confess that the lives' work of Thomson and Tait has been a total failure as regards its influence on the truly practical men of their generation. Can this be so?

Prof. Greenhill's remarks on the abominable semi-numerical equation $W = Mg$, or $\frac{W}{g} = M$, I most heartily welcome [where W is a mass and g a numeric; the moment writers on dynamics, who use the gravitation system, pass from merely proportional equations to their physical interpretation, then must we face with them this most wretched equation]. But perhaps it will be at once a surprise and a gratification to Prof. Greenhill to know that a whole (academic) generation of Scottish University engineering students has been taught to eschew this same equation as an unclean thing, and to adopt the mode of thought clearly set forth in the last two paragraphs of his letter of February 28. Some five or six years ago I was myself so taught by one who is now, alas! no more. The possibility of thus clearing of cant not only the engineering but the purely mathematical mind seems to me, as indeed Prof. Greenhill indicates, to be a direct consequence of the acceptance by Thomson and Tait of the British Imperial pound as the unit of mass or quantity of matter.

Having fixed, then, for good and all, the unit of mass, and taking the British foot and the second as the units of time and length respectively, the unit of force defines itself in virtue of Newton's Second Law. To this unit—the British unit of force—Prof. James Thomson has given, as nearly everyone knows, the name "poundal." Now the most convenient practical unit of force, for physicists as well as engineers, is not the poundal, but the gravitation at the earth's surface of the unit of mass, a quantity which is not absolutely constant, the inconvenience so arising being, however, practically unimportant, or at most involving a reduction to an arbitrary standard. To pass, then, from a force expressed in poundals to a force expressed in units of gravitation of the Imperial pound at the standard place, one simply wants to know how many poundals go to the gravitation of the Imperial pound at that place—in other words, the change ratio. The answer is simple: the numeric g for the standard place; for a force equal to the gravitation of the Imperial pound at the standard place acting upon a mass of one Imperial pound would generate a momentum per second of g (numeric) pounds mass × feet per sec. per sec.; and the poundal a momentum per second of one pound mass × feet per sec. per sec.; and by Newton's Second Law the ratio of the forces is, therefore, the numeric g. What more does the physicist or engineer want to know? How many poundals go to the gravitation of a ton mass at the standard place? Answer, the numeric 2240 g. Could anything be simpler? The difficulty is to find the difficulty, or to assign the *raison d'être* of the so-called gravitation unit of mass.

When the old unnatural gravitation unit of mass is abandoned, and the transition from the natural unit to the gravitation unit of force made by means of the change ratio, the vicious use of the word mass to denote the result of dividing by the numeric g the mass of a body in standard pounds (viciously on the same system called the weight and denoted by W) ceases; and the dire confusion between weight and mass becomes a thing of the past. The emancipation of the term weight from its bondage to mass would appear to have afforded opportunity for its use wherever it might be of service in suggesting or denoting gravitation. For example, there are three units, each called a pound, viz. the Imperial British unit of mass, the gravitation of the same mass at any the same place on the earth's surface, and the pound sterling. Physicists, like Sir Wm. Thomson and Prof. Tait, use terms such as "pound weight," "gramme weight"; similarly we have "pound mass" coming into use; and probably we shall soon hear of "pound money." Prof. Greenhill, with his strong engineering sympathies, objects to the time-honoured "pounds per square inch" being rendered "pounds weight per square inch"; and if I may presume to offer an opinion, the old phrase is already cumbrous enough. Still, if Prof. Tait and Prof. Greenhill ultimately agree that there is anything to be gained in perspicuity, for the sake of the weaker members perhaps it might be well for us to put in practice, on some occasions, the injunction to sacrifice all rather than cause our brother to offend.

Some one might possibly step in to draw attention to the fact that the pages of even our own great high priest of exact applied science are disfigured by $\frac{W}{g}$; but sure I am that were Rankine now with us he would lead the way with Prof. Greenhill in a crusade against the apologists for the obscurity of which W/g is the symbol. Take, for example, Rankine's bold introduction of the dynamical unit of quantity of heat. Take the opinion of

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Clausius, than whom none could be a more unprejudiced witness. "At the same time in the theoretical development of the mechanical theory of heat, in which the relation between heat and work often occurs, the method of expressing heat in mechanical units effects such important simplifications that the author has felt himself bound to drop his former objections to the method on the occasion of the present more connected exposition of that theory." ARCHD. C. ELLIOTT

Edinburgh, April 18

Seismometers

I HAVE long ago learnt not to look for any fair recognition of my work in seismometry on the part of Prof. John Milne, and when he accuses me of appropriating without acknowledgment the work of others it is time to decline further controversy with him. The points raised in his last communication (NATURE, April 14, p. 559) are sufficiently answered in mine of December 11 (p. 172). I there quoted part of a letter written by Prof. Chaplin, now of Harvard University, then of Tokio, and Secretary of the Seismological Society of Japan, under whose eyes the events occurred to which Prof. Milne refers. I did not quote the whole of Prof. Chaplin's letter, because it contained sentences I was unwilling to give except under the strongest provocation. After referring to my seismograph in the words already quoted (p. 172), Prof. Chaplin continues:—

"I do not remember that in the discussions on your machine Mr. Gray ever claimed to have invented a similar machine, and I am surprised to know that he makes that claim now. On this and other points it appears to me that Messrs. Gray and Milne have not treated your inventions and investigations with fairness, and that you have just grounds for complaint. I am willing you should make such use of this note as you see fit."

As to the question of priority, this judgment, from a man at once unprejudiced, most competent to form an opinion, and fully informed of the matter in dispute, must (so far as I am concerned) close a discussion of which your readers cannot but be weary. With your permission I shall give, in a later number of NATURE, an example of the excellent work which Prof. Sekiya is now doing with my instruments in Japan.

J. A. EWING

University College, Dundee, April 16

April Meteors

THE Lyrids have, this year, offered a somewhat scanty display, though a few brilliant meteors have been seen shooting from the usual radiant-point.

In 1884 April 19, this shower was very rich, the horary number of its meteors for one observer being about 22, but in the following year, 1885, it exhibited a considerable decline, the hourly rate being only 3. In 1886 I obtained no observations, owing to the bright moonlight and in the present year, on April 20, the horary number was slightly more than 2, so that the numerical character of the recent display has fallen far short of some of its apparitions in preceding years.

On the night of April 17, this year, the shower had not visibly opened, for none of its meteors were recorded in a 2½ hours' watch. On each of the nights of the 18th and 19th the sky was closely observed for 4½ hours, but the Lyrid shower was very feeble, and only furnished 1 meteor per hour. On the 20th, in 3 hours I noted 7 Lyrids, and these were brilliant.

The average radiant-point from the three nights was at 269° + 32°, and there is confirmation that this point advances in R.A. with the time, though not to the marked degree ascribed in NATURE for May 7, 1885, p. 5. But the meteors from this stream have been so scarce at their late recurrence that it has been very difficult to ascertain the exact radiant for each night. Moreover, these Lyrids move with great apparent velocity, flashing out with extreme suddenness and they are gone, together with the faint streaks sometimes accompanying them, before the eye is enabled to catch the directions with satisfactory precision.

On the four nights April 17 to 20 inclusive, I noticed 70 shooting-stars belonging to the minor systems of the Lyrid meteoric epoch, and amongst these the best was that of a radiant of very swift, short meteors at 231° + 17°, a few degrees west of β Serpentis. This stream is not new, for I saw a well-defined shower of Serpentids from the same point during my observations of the Lyrids in 1885, on April 19–20 (NATURE, May 7, 1885, p. 6).

In this and in previous years I have also recorded some meteors ascending in very long flights from a radiant centre close to θ Libræ, at 235° − 15°. This is the only observation of this shower at the April period, though Lieut.-Colonel Tupman found a pair of radiants near the position assigned in the first week of March 1869–70.

I subjoin a short list of bright meteors seen here while watching the progress of the Lyrids, and I should be glad to hear that any of these had been observed elsewhere.

Date	Hour	Mag.	Apparent Path		Notes	Radiant
			From	To		
1887	h. m.		° °	° °		
April 19	13 13	♀	269 + 11	269 + 1	Swift, streak	Lyrid
19	13 46	♃	308 + 61½	56 + 65½	Swift, streak	279° + 13°
20	9 48	1	211 + 7	194 − 4	Very swift	Lyrid
20	10 29½	>1	243 + 14	234 + 5	Swift, streak	Lyrid
20	10 47	1	239 + 53	269 + 62¼	Slow, train	206° + 18°
20	12 28	>1	308 + 40	316 + 40¼	Very swift	Lyrid

Bristol, April 22 W. F. DENNING

Vertical Decrement of Temperature and Pressure

IN NATURE of March 10 (p. 437), Mr. Maxwell Hall gives an interesting table of the vertical distribution of temperature and pressure in Jamaica, and, apparently in happy ignorance of the dangers of the process known as extrapolation, goes on to apply the results of observations extending to a maximum height of only 7400 feet to the determination of the probable temperature of meteorites in extra-terrestrial space. As he expresses a desire to know whether any similar results have been found in India, and as I have on several occasions during the past ten years discussed the vertical distribution of temperature and pressure in this country, I gladly take this opportunity of referring him to my papers on the meteorology of the North-West Himalaya, and on the temperature of North-Western India, published in the "Indian Meteorological Memoirs," vols. i. and ii. From the latter I extract the following table on the mean decrement of temperature up to a height of 12,000 feet, computed from the observations of twenty-five stations combined in various ways. For each month an interpolation formula of the form

$$T = T_0 + ah + bh^2 + ch^3,$$

was computed, and by its means the decrements from sea-level to 1000 feet, 1000 to 2000 feet, &c., were calculated. Finally, the average decrement for the twelve months was computed, and is here given in an abridged form. The curves for the several months differ very widely from one another, those for the summer giving the most rapid decrement at sea-level, and the decrement increasing with altitude in winter:—

Height Feet	Mean temperature decrement ° F.
0 to 2000	6·16
2000 to 4000	5·87
4000 to 6000	5·61
6000 to 8000	5·37
8000 to 10000	5·16
10000 to 12000	4·98

The mean height of the barometer at sea-level in the region in question, the centre of which lies a little north of Simla, is about 29·8 inches; the mean at 6000 feet is 24·1 inches, and the mean at 12,000 feet about 19·4 inches. With these data, and adopting Mr. Hall's formula

$$\delta T = \lambda.\delta P + \mu.(\delta P)^2,$$

we find $\lambda = 2^\circ{\cdot}979$ and $\mu = 0^\circ{\cdot}02$. These coefficients do not differ widely from Mr. Hall's values, which are 2°·92 and 0°·08 respectively. At the limit of the atmosphere, where $\delta P = 29''{\cdot}8$, δT would be − 106°·5, which would give, as the mean temperature of external space, about − 30° F., the mean temperature at sea-level being 77° F.

Taking the simplest formula, $\delta T = \lambda.\delta P$, we find $\lambda = 3^\circ{\cdot}19$, which is almost identical with the value quoted by Mr. Hall from an early volume of NATURE, but which, if it held good to the limit of the atmosphere, would make the temperature of external space about − 18° F., since the mean temperature at sea-level is 77° F.

The only conclusion to be drawn from such observations is that the vertical decrement of temperature on mountains varies greatly with locality as well as season, and the results

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